Response to Anonymous Referee#1 comments for "The circulation of Icelandic waters – a modelling study" by K. Logemann et al.

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We would like to thank the anonymous reviewer for her/his comments which helped us to clarify the aspects of the paper which have been ambiguously before. The reviewer's comments are displayed with a bold/italic font.

The authors combine a relatively high resolution regional ocean circulation model with assimilation of CTD data to simulate the currents and hydrography around Iceland for the period of 1992 to 2006. The model is forced with surface fluxes derived from NCEP atmospheric variables using bulk formulae, and river runoff. A detailed description of the resulting currents, and a broad comparison with observations, are given. Some sensitivity calculations are carried out in order to infer the driving mechanisms for various currents. Several new currents are identified and given names. The primary focus of this paper is to try to produce the most realistic simulation of the ocean and describe the result. As best I can tell, the authors seem to be successful at that.

Thank you.

However, the model does not really provide any definitive answers to dynamical questions about what determines aspects of the flow, such as volume fluxes, heat transport, etc.

We agree that within the non-linear, chaotic, coupled ocean-atmosphere system models of linear causality always have a limited range of validity. As the similarity between the simulated and the observed structures is not contradicted by the review we assume that the negation of any real knowledge acquisition points to the series of sensitivity experiments and their interpretation. We do not share this view but we recognise that our formulations using the Artic Front as a cause of ocean currents may be misleading because the front is also a result of the flow field. (Though, the NIIC and SIC are driven by the barotropic pressure gradients related to the Arctic Front, i.e. they depend on the Arctic Front, but the Arctic Front does not depend on the NIIC or SIC). In the revised version of our manuscript we refer, more precisely, to basin-scales differences of the sea level height caused by basin-scale density differences. (More discussion about the sensitivity experiments below.) We also have extended chapter 4 which describes the sensitivity experiments.

There is some attempt to infer dynamics through the sensitivity calculations, but I think the results are ambiguous for reasons detailed below.

Let us assume a point within the ocean where we measure a current which reverses around four times per day. Let us assume there is an ocean model which is able to essentially reproduce this behaviour. Then, we eliminate the terms of wind stress from the model equations. Thereafter horizontal density gradients, but the cyclic current persists only slightly altered. Finally, we eliminate the sun's and moon's gravitation from the model equations and this leads to a complete shut-down of the simulated current. We could then say, that our model experiment indicates a high probability for a tidal cause of the observed structure. But, of course, the certainty would not be 100%, though very close to. Our analysis of the various sensitivity experiments uses this chain of thought, the results may be not always as evident as in the tidal current example above, but we cannot find a crucial systematic error here. (More discussion about the sensitivity experiments below.)

The discussion is quite descriptive and, I think, intended to provoke new analysis of observations and perhaps more focused dynamical studies. There is nothing obviously incorrect in the paper, but I see this largely as an editorial decision as to whether or not such model descriptions are useful.

The reviewer is correct in her/his analysis that one focus of the paper is the description of the numerical solution. Once again we would like to emphasise that this solution contains the information of 16,802 CTD profiles, furthermore it contains the Icelandic river runoff simulated by the WaSiM model which is based on a huge data base of hydrological observations made in Iceland, and thousands of bathymetric and meteorological observations as well. The presented solution resolves the three-dimensional oceanic space with an unprecedented high spatial resolution. It proposes mean circulation structures which are partly new and partly may lead to more clarity regarding ocean areas which were described in the past by different schemes which are, despite the inconsistency between them, still widely used.

The kind of ocean data product presented in this paper is nowadays used in marine biology and meteorology, within the fishing industry and for nautical navigation up to yachting. Why should oceanography be uninterested?

The above cited NCEP/NCAR re-analysis fields are created by a computer model and assimilated observations as well. Their importance for modern geoscience is beyond discussion.

As an aside, I would prefer that new currents are identified and named based on observational evidence, not model results.

We replaced the words "South Icelandic Current" by "hitherto unnamed current" in the abstract in order to reduce the impression of importance related to this naming issue. However, within the paper we needed some kind of names for all currents because thoughts can be hardly expressed without them and the compiled tables need some kind of printed symbol at the end of the row or column. We thought about using numbers but assumed this to cause confusion. At the end of the paper we write that "these postulates require observational verification". Within the revised version of the paper we will emphasise even more forcefully that "simulated currents" are discussed.

There are numerous aspects of the paper that need improvement. The model description is inadequate, the figures need improvement, and some of the analysis is misleading.

Will be discussed below.

Many of the references are in the gray literature and thus will not be accessible to many readers.

When writing the manuscript it was clear to us, as stated within the OS guidelines for authors, that we had to minimise the number of grey literature to the inevitable. Numerical models are often described and documented with extensive technical reports which quite definitely makes sense. Furthermore we think that the two Icelandic master theses had to be mentioned.

If the editor feels that this sort of descriptive numerical oceanography will be of interest to the journal readership, it could be published subject to major revisions.

Detailed comments follow.

(page 766, line 7) State where the Atlantic Water moved into the Nordic Seas.

Description of the main pathways added

(766, 13) volume flux volume flux

corrected

(766, 16) The Arctic Waters are also present north of the ridge to the west of Iceland.

Which is written in line 22-23. Line 16 describes the EIC.

(767, 12) What atmospheric processes? This statement is very unclear.

Good point. The sentence now mentions the wind field anomaly with strong northerly wind north of Denmark Strait

(769, 22) coastal

corrected

(770, 15) define d/dt

This part of the manuscript was removed.

(771, 1) What is the lower boundary condition for w?

This part of the manuscript was removed.

(771, 5) w(z=zeta)?

This part of the manuscript was removed.

(772) An ice model is mentioned later, but there are no details. What are the dynamics and thermodynamics?

A further paragraph about the sea ice model including the Hibler (1979) reference was added to chapter 2.1

Section 2.2.1: How is a cell divided into 8 parts if each side is halved? Isn't that 4 sub-cells?

No, they are 8 because it is a three-dimensional refinement and 2^3=8. We have changed sentence (773, 17) in order to make this clearer.

How is it determined if finer resolution is required? What is the topographic criteria?

We have changed to term "topographical criteria" to "geographical criteria" in order to clarify that we did not use criteria like a certain ocean depth gradient but rather the geographical criterion: "The closer to the Icelandic coast the higher the resolution" which is described at the end of chapter 2.2.1

It would be clearer to show a contour plot of the horizontal grid spacing rather than the grid cells, I cannot tell what the resolution is around Iceland. The axis labels are way too small on Fig. 2.

We replaced Fig. 2 with a graphic showing the three-dimensional computational mesh with more clarity, including different colours for different resolutions.

I do not understand the sentence beginning "By using a mix..."

We removed the imprecise term “mix” and now refer to the latitude dependent weighting function for the different projections.

(774,12) Does "this month" refer to days 1-30, or from the 15th of the previous month to the 15th of the current month?

Now we state that data from the 1st to the 30th of the month are used.

(774,16) Why mention all the error terms (e.g. u, v, w, mixing coefficients) if only the heat flux terms are used (line 25)?

Please observe that we used the word “dominant”. All error terms are related to heat fluxes, but only the term which is related to the dominant heat flux is chosen for correction.

How does one determine if the errors are due to advection or mixing? Does it matter?

The model is not able to determine the source of the error. It assumes the error to be at the term with the greatest absolute value. First, this assumption is not as incongruous, second the correction term’s efficiency is guaranteed this way.

Are the corrected heat flux terms three dimensional? If so, it is misleading to call them a heat flux since this is generally taken to mean heat exchange with the atmosphere.

We do not think that a “heat flux” is always an air-sea heat flux. E.g. the widespread term “meridional heat flux” then would be a paradox.

(775,2) Please show some measure of convergence. Is three iterations enough?

Now, the decrease of the mean deviation between model and CTD data by one order of magnitude after the third iteration is shown.

The description of the correction term (lines 2-5) is very unclear, please expand.

We have added some more text and the reference to Logemann et al. (2012) where the computations are given with all details, which are beyond the scope of the paper.

(775,15) I assume some bulk formulae are used to get surface fluxes from atmospheric variables?

Yes, as stated in Chapter 2.1

(776,11) How is the river runoff imposed on the model? Is there a volume flux at the coast? If so, provide details. How is this volume flux balanced over the whole domain? Is the model domain gaining volume or is it taken out of the domain somewhere?

We have added: “The discharge is simulated by prescribing the according rise of the sea surface and decrease of salinity for the model cell being closest to the river mouth. The resulting gain of mass of the entire model system is balanced by a sea surface elevation correction term being evenly spread over the entire model domain.”

(777,6) How large are the correction terms in the data assimilation compared to, say, the local surface fluxes or the nonlinear advection terms?

Good point. We have compared the freely forecasted monthly temperature and salinity change in Icelandic waters with the monthly change computed including data assimilation. So, if the model would just be a “glorified interpolator”, i.e. completely unable to freely reproduce any physical process, the portion of freely forecast change should be close to 0%. However the median portions are 91% for temperature and 89% for salinity. We will include this information in the manuscript.

(778,5) Which tidal components were used in the model?

Now we state in chapter 2.1 that we use a “tidal potential, given by a first order approach (Apel, 1987). Here, we set the solar and lunar co-declinations to time invariant constants which reduces the tidal spectrum mainly to the M_2 and S_2 constituents (Logemann et al. 2012)”

Figure 5 (and vectors) is much too small to be useful.

We have increased the resolution of the graphic and increased the size of the vectors, numbers and letters.

(778,12) The assimilation of hydrography would be expected to improve the velocity field if the flow is near geostrophic, which it is, so this improvement is not surprising.

We agree, the sentence was removed.

(778,25) I do not understand the sentence starting "Here, the temperature..."

We agree, the sentence was redundant and was deleted.

(780,7) Please label or provide some guidence as to where these geographical features are.

Now, we refer to the Fig. 3.

(782, 9) I am not convinced the branching of the oNIIC is robust. This is especially a concern given the co-location of fronts with step changes in the model topography (most evident in Figs. 11 and 12).

Now, we state that a better resolution of topography could alter the shape of the oNIIC branching.

(782, 17-25) The NIJ appears to be too deep in the model, the observations have it located over the 650 isobath. Also, the model shows an increase in westward transport as one moves to the east while the data show a significant decrease in westward transport. This is a major disagreement with the observations that needs to be made clear.

Now, we point to that.

(783, 10) Should be Fig. 6?

Yes, corrected.

(784, 9) To whom does "they" refer?

The "the results" – the sentence was reformulated

(784, 14) What does a 1 percent model error mean? I would be surprised if the model were within 1 percent of the observations. What are the error bars on the observations?

Good point. Now we compare the absolute values, including the uncertainty of the observational based value.

(784, 20) What is the correlation and significance between these two time series?

Now, we also state that the correlation coefficient (Pearson's) is 0.77.

(785, 1-9) Lots of weakly supported speculation here.

We removed the speculation about the SIC 2005 minimum. We removed all kind of discussion from chapter 3. We stress that the massive SIC AW flux between 1992 and 1999 is a model result.

Section 4: Why pick the 6 month period with the largest change in circulation to infer what is driving the mean circulation? This seems like just the wrong time period to focus on.

During the second half of the year 2003 we see indeed a quite significant decrease of the observed NIIC volume flux (around 20%). However, the simulated NIIC remains rather constant during that period and also the variations of the other currents are rather in the normal range. Furthermore, within our sensitivity experiments we analyse the conditions for a complete collapse of the given current. A volume flux change in the range of ± 20% during that period is irrelevant in this context, because it does not point to a deactivation of a crucial forcing process. I.e. it has no consequences for the results of the sensitivity experiments.

What is the time scale for information to propagate from the edge of the circular perturbation region to the coast of Iceland? Just because you did not find a sensitivity in 6 months does not mean that there is no sensitivity, it may not have arrived yet. For example, it would be wrong to conclude that the wind stress curl in the eastern portion of a subtropical gyre has no influence on the western boundary current transport just because it takes some time (basin width / Rossby wave propagation speed) for the influence to get there.

These time scales vary from a few minutes up years. We agree, that the ocean structure around Iceland is influenced by an endless number of processes which partly operated far away and long time ago. How could we explain the meridional temperature gradient without the sub-tropical absorption of solar radiation and the ocean heat emission in the Artic? How could we explain the northward flow of Atlantic Water and the southward flow of Arctic and Polar waters, how the different water mass properties within the different ocean basins without referring to the Atlantic general circulation? Maybe our chain of causality would end with the nuclear fusion deep within the sun.

However, our question is directed differently. We look at a certain current in Icelandic waters and ask: What is the immediate, direct process which forces this movement? Wind stress? Tides?

baroclinic pressure or sea level elevation gradients? What is the first link of the chain of causality? And we are interested in this first link because we try to understand the local physical system of Icelandic waters with its interacting processes.

The applied method is not as blind as the reviewer assumes: If the local area would be shifted to the North America East Coast, and if the analogue sensitivity analysis would be carried out with the Gulf Stream, then none of our experiments would lead to a collapse of the current. So, more or less as the last option, the experiment “no wind in the entire model domain” would be performed. This would finally, maybe it would take longer than 6 months, lead to a drastic drop of the volume flux and the basin-scale wind field would be identified as the basic forcing mechanism, as the first link in the chain of causality. However, if we would be able to simulate an immediate Gulf Stream collapse just by switching off the wind over Iceland, we could deduce that something related to the Icelandic wind field is forcing the Gulf Stream.

I am not convinced that the "no horizontal density gradient" calculations are useful. The real question is what determines the horizontal density gradients, the flow will adjust to the baroclinic shear. All these calculations tell us is whether the flow is baroclinic or barotropic.

This point is hard to understand: It is rather unimportant to know whether the currents are forced by (or related to) the local or basin-scale density gradients or not? But the real question of the dynamics of Icelandic waters is why the Arctic waters are cooler than the Atlantic Water, or which processes are leading to the Arctic Front? Maybe the reviewer wants to point to the fact that a density front is not just causing the adjusted baroclinic shear but has to be formed by a convergent current field. However, we want to know whether the density field, local, or basin-scale is related to the specific current we have simulated. We have added a paragraph to chapter 5 discussing the problem of defining the causality of flow in a stratified ocean.

(788,9-15) The vectors are too small to be useful.

The vectors size was increased.

I do not believe these calculations are steady, or dissipation must be very large.

It has to be considered that the density field is stationary. The shallow water wave caused by the spin-up was considerable damped two days after the spin-up.

It is surprising you do not connect the deep counter current with the NIJ, which is this model’s version of the Vage et al. mechanism.

Of course, the sentence “A counter-current is found in deeper layers (Fig. 15d).” points towards the NIJ. But here we discuss the general solution of the zonal flow along a vertical, stationary density front. Our model experiment is similar to that of Hsieh and Gill (1984), with the exception that we added a circular island on the front and prescribed a stationary density field.

The model experiment of Våge et al. also uses a circular island and they refer to the same region, but these are basically the only similarities to our experiment. Våge et al. include (1) a zonal sill that separates a southern gyre from a marginal sea to the north; (2) buoyancy loss in the marginal sea and (3) cyclonic wind stress curl. We are not sure what the reviewer means with “Våge et al. mechanism”. Våge et al. do not perform sensitivity experiments, do not write about the NIJ forcing but describe the formation of NIJ water by mixing of NIIC AW with the AIW of the Iceland Sea. However, the water mass formation of the NIJ water or DSOW was beyond the scope of our paper.

We added to chap. 5. That already Hsieh and Gill (1984) described the basic NIJ forcing.

(791,4) What is "The theory of secondary circulation"? There is more than one.

Now it is: “The theory of secondary circulation related to Ekman-layer dynamics (e.g. Holton, 1979; MacCready & Rhines, 1993) says…”

(791,24) Was there a shelf in Fig 15? If so, its influence on the circulation was never discussed. I would expect a similar result with no shelf.

Good point. The topography is shown in Fig. 15a, also having in mind Eq. (11) with the Tangens Hyperbolicus, it should be clear that there is a reduction of the water depth around the island. The influence of the shelf on the NIIC/SIC structure is now examined by two further experiments (Chap. 4.1, Fig. 18). It shows that the existence of the shelf is a necessary condition for the NIIC/SIC.